Art. I.—*Observations with Kater's Invariable Pendulums made at Sydney during January and February, 1894; with an Appendix on the Stability of the Pendulum Stand.*

(With Diagram).

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[Read 8th March, 1894.]

Introductory.

The object of this investigation was to throw some additional light on the question of the difference between the values of g at Melbourne and Sydney. Two determinations of this difference had already been made; the officers of the United States Coast Survey swung the Kater pendulums at Sydney in 1883, these pendulums being also swung by Mr. Baracchi at Melbourne in 1893; while Lieutenant Elblein swung three of von Sterneck's pendulums at Melbourne and at Sydney in the winter of 1893. When these two sets of results came to be compared,* they were found to be inconsistent; the U.S. Coast Survey figures, combined with those of Mr. Baracchi, show that a pendulum beating approximately seconds should lose 8·58 vibrations per day, if transferred from Melbourne to Sydney; while Lieutenant Elblein's figures give 13·48 as the loss per day. I accordingly decided to swing the Kater pendulums again in Sydney at the earliest opportunity; and, as a matter of fact, the observations in Sydney succeeded those of Mr. Baracchi in Melbourne by a little more than three months. During the interval I made a few measurements in Melbourne; these agree in the main with those of Mr. Baracchi, but are so much less elaborate that there is no need to publish them. The observations in Sydney follow my own in Melbourne at an interval of five weeks. We may therefore reasonably consider that the comparison between Melbourne and Sydney recently secured lacks nothing in point of directness.

* Baracchi—Proc. Roy. Soc. Vict., 1893, p. 176.

Arrangements.

The pendulums and subsidiary apparatus* were carefully packed at the Melbourne Observatory and shipped to Sydney, whither I proceeded on 19th January. Mr. Russell, the Government Astronomer at Sydney, had very kindly placed at my disposal the cellar in which the experiments of the U.S. Coast Survey party, and subsequently those of Lieutenant Elblein, had been carried out; and as the exact position of their apparatus in the cellar is known I erected mine on the same spot. The cellar itself is almost an ideal room for the purpose. Three of the walls are of brick; one, which is two feet ten inches thick, is directly in contact with the earth outside, forming part of the foundation wall of the Observatory; the other two, which are two feet four inches thick, form partition walls separating the room from adjoining cellars, as does also the fourth wall, which is a mass of stone four feet three inches thick, and supports the Transit instrument. At either end of the Transit wall are narrow passages communicating with the adjoining cellar. The ceiling, which is of wooden panels, is level with the ground outside. There are no windows; but at the east end—remote from the pendulum apparatus—a staircase leads up into the Transit room. The dimensions of the cellar are twenty-four feet by six feet five inches by seven feet seven inches. As might be expected from this description the diurnal variation of temperature cannot be detected in this room, even by experiments specially carried out for the purpose.†

The floor, on which the pendulum stand was erected, consists of six inches of concrete resting directly on a bed of very hard clay containing a large number of iron stone nodules. This clay bed, which is nearly one foot thick, is in its undisturbed natural condition and very solid; it rests directly on the Sydney sandstone. A method of testing the stability of the apparatus—and of the floor too—is given in the Appendix.

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* Described in the Report of the Gravity Survey Committee for 1892—Proc. Roy. Soc. Vic., 1892, p. 219.

† I kept the thermographs running whether I were at work or not; on certain days no one entered the cellar, and the records for those days are straight lines. A range in temperature of 0·1° Fahr. could be detected at once by the wave it would produce in the line; but none such was found.

The Shelton clock was supported by a couple of $\frac{7}{8}$in. planks, each of these being secured by large screws to four plugs inserted about eight inches into the wall at the west end of the cellar. The clock was attached to the planks by three screws, and set vertically by inserting mahogany wedges between the planks and the clock case. The verticality, as tested by the spirit level attached to the clock, was well maintained during the whole series of observations.

The relative positions and distances of the apparatus were identical with those employed in Melbourne,* save as regards the position of the observing telescope (*vide infra* p. 5). The operation of inserting the pendulums into the cylinder was considerably simplified by cutting holes in the ceiling of the cellar, and in the floor of the room above; Mr. Russell would then hand the pendulum down through the hole, I receiving it below and guiding it into the cylinder; in this way the pendulums, when not lying in their boxes, were always kept in a vertical position and supported by their upper ends, so that risk of accidental bending was practically eliminated. The uppermost of the two holes, when not in use, was kept closed by a board chamfered to fit its edges, and above this again was a sheet of linoleum; no draught or air circulation through the holes was ever detected during the swings.

Pressure and Temperature.

The experiments were carried on under atmospheric pressure, the pressure being recorded by a marine barometer lent me by Mr. Russell; the cistern of the barometer was placed approximately on a level with the bob of the experimental pendulum. The barometer corrections are given in Table I.

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* Baracchi, *l.c.*, p. 164-66.

Table I.

COPY OF CERTIFICATE OF MARINE BAROMETER, C758, ISSUED BY THE KEW (ENG.) OBSERVATORY.

At	in. 27·5	in. 28·0	in. 28·5	in. 29·0	in. 29·5	in. 30·0	in. 30·5	in. 31·0	Correction to attached Thermo'ter at 62°	Time of falling one inch.
	−0·010	−0·009	−0·007	−0·006	−0·005	−0·003	−0·002	−0·001	+0°.3	5 min. 50 sec.

These corrections include those for index error capacity and capillarity.

(Signed) G. M. WHIPPLE, *Supt.*

Note.—Compared with the standard in Sydney, the error was 0·001 greater.—H. C. R.

The temperature was determined as in Melbourne by means of the thermometers, K667 used in the inverted position, and K668 in the erect position, both attached to the dummy; and as a check on any irregular variations of temperature, the two Richard thermographs were employed. The tracings furnished by these were in all cases so regular, that the mean of the thermometer readings, with the correction $-0^\circ 13$ applied, could be always taken as representing the mean temperature of the pendulum with sufficient accuracy. The thermometers and barometer were read before and after the observations at the beginning and end of a set of swings; and each recorded reading is therefore the mean of four observations. The fluctuations in barometric pressure were also observed by means of the Observatory barograph; though not large enough to sensibly effect the pressure correction, they influence the observations in another manner, as described in the concluding paragraph of this paper.

Arc of Vibration.

This was read on the arc scale behind the tail-piece of the pendulum, as in previous observations with this apparatus.

Levelling.

The agate planes of suspension for pendulums No. 4 and No. 6 were adjusted to horizontality with the aid of two small but very sensitive levels sent out with the apparatus; each of these stands on three sharp points. The agates belonging to No. 11, being cylinders instead of planes, could not be adjusted with these levels; the two flat-based levels sent out with this pendulum are very sluggish, and not very sensitive; I accordingly employed a very delicate flat-based level, kindly lent me by Mr. Ellery. The planes generally remained in good adjustment as tested by relevelling at the close of the series for each pendulum.

Observation of Coincidences.

In setting up the apparatus the observing telescope had to be rotated to the left of the vertical, so that the observer sat with the pendulum stand on his left. The disappearance and reappearance of the apparent left edge of the image of the disc on the clock pendulum were in every case selected for observation; this edge would be the apparent right edge to an observer on the opposite side of the room, and consequently was the same as that observed in Melbourne. The card disc on the clock pendulum was that used in Melbourne, and the method of observing was the same as that adopted in Melbourne, Kew and Greenwich. Four sets of swings, two for each face, were effected with each pendulum, the two sets for any one face being taken on the same day. The discordance between the results for opposite faces with the Pendulum No. 11* was very marked; but there is no doubt that this pendulum is slightly bent, and very little question that its knife-edges are not accurately perpendicular to the pendulum bar. I noticed both these defects on the first arrival of the pendulums from England. Fortunately, so long as they are constant they do not affect the accuracy of differential observations.

Clock Rates.

The Shelton clock was compared directly with the Siderial clock of the Observatory at the beginning and end of each day's work, in order to determine the difference of their rates. The comparison was effected by means of a tape chronograph of Morse's pattern, constructed by Messrs. Siemens Bros., which worked very uniformly. The chronograph spaces were measured off by means of a divided lens, the halves of which were mounted on brass sliding pieces carrying scales; this instrument being used in much the same way as the heliometer. The scales were graduated in inches and tenths, and hundredths were estimated.

The error of the Siderial clock was determined by Transit observations. Unfortunately the nights were so cloudy for most of the time that star observations could not always be obtained, and sun transits had perforce to be resorted to; this cannot, however, have affected the results to any serious extent, as on those occasions when both sun and stars were observed the difference of the deduced rates was never more than one or two hundredths of a second. Both clocks behaved well; their rates are given in Table II.

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* Alluded to by Baracchi, *l.c.*, p. 166.

TABLE II.

Date.	January.			February.				
	29th	30th	31st	1st	2nd	3rd & 4th (Mean).	5th	6th
Rate of Siderial Clock belonging to the Observatory - -	−0·54	−0·54	−0·50	−0·43	−0·63	−0·72	−0·54	−0·50
Rate of Shelton Clock - -	+7·18	+7·06	+7·06	+6·99	+6·62	+7·12	+7·10	+7·12

The Observatory Siderial had, as the table shows, a losing rate, and the Shelton clock a gaining rate, throughout the series.

Reduction of the Observations.

This was done in the manner (now well-known) adopted at Greenwich, Kew, and Melbourne. The method of reduction can be easily understood with the aid of the following notation :—

I = duration of a set of swings.

n = number of coincidences in a set.

N = interval between two consecutive coincidences.

N_0 = approximate value of N.

R = number of siderial seconds in a solar day increased by the rate of the Shelton clock = 86636·56 + rate.

B = mean barometric pressure (corrected).

T = mean temperature (corrected).

a, b = initial and final amplitudes in inches.

D, d, r = distances of arc scale from the telescope, tail-piece and knife-edge respectively measured in inches.

V = vibration number.

V_0 = approximate value of V.

I is obtained by subtracting the epochs of the first three from those of the last three coincidences and taking the mean of the differences : N_0 is observed directly during the experiments, and n is obtained by dividing N_0 into I, being the nearest whole number to the quotient : $\frac{I}{n}$ then gives N.

$$\text{Pressure correction} = 0{\cdot}34 \frac{B - 26}{1 + 0{\cdot}0023\ (T - 32)} = \beta.$$

$$\text{Arc correction} = V_o . \frac{\overline{D - d}^2}{16 r D} . \left\{ \overline{a + b}^2 - \tfrac{1}{3} . \overline{a - b}^2 \right\}$$

$$= 0{\cdot}13 \left\{ \overline{a + b}^2 - \tfrac{1}{13} . \overline{a - b}^2 \right\} = \alpha.*$$

$$\text{Temperature correction} = 0.45\ (T - 62) = \tau$$

$$\therefore V = R - \frac{2R}{N} + \alpha + \beta + \tau.$$

The reduction is to standard temperature 62°F., and standard pressure 26 inches of mercury.

The results are given in Table III.

* The coefficient 0·13 is calculated from the following approximate values :—$D = 71$, $r = 50$, $d = 1$, $V_0 = 86000$.

Table III.

Pendulum.	Date.	Face.	Clock Rate.	R In Seconds.	I In Seconds.	n	N In Seconds.	B In Inches.	T In Deg. Fahr.	a	b	$R-\frac{2R}{N}$	α	β	τ	V	Mean for Face.	Mean for Pendulum.
No. 4	Jan. 29th	M.	+7·18	86643·74	16364·3	53	308·76.	29·895	69·62.	0·530	0·105	86082·504	+0·058	+1·191	+3·429	86087·182	86087·16	86087·10
					15127·3	49	308·72	29·890	69·70	0·585	0·130.	86082·431	+0·057	+1·190	+3·465	86087·143.		
	Jan. 30th	P.	+7·06	86643·62	12355·7	40	308·893	29·828	69·60	0·645	0·195	86082·586	+0·083	+1·198	+3·420	86087·287	86087·04	
					16355·7	53	308·598	29·786	69·82	0·585	0·115	86082·051	+0·054	+1·184	+3·510	86086·799.		
No. 6	Feb. 1st	P.	+6·99	86643·55	13370·0	51	262·157	29·673	69·79	0·555	0·150.	85982·545	+0·050	+1·190	+3·506	85987·291	85986·88	85987·12
					15992·5* 15993·5 16257·5	61 61 62	262·193	29·699	69·90	0·540	0·095	85982·635	+0·044	+1·198	+3·555	85987·432.		
	Feb. 2nd	M.	+6·62	86643·18	14681·3	56	262·166	29·776	69·61	0·505	0·105	85982·201	+0·041	+1·182	+3·425	85986·849	85987·36	
					16516·0	63	262·159	29·804	69·75	0·565	0·100	85982·182	+0·048	+1·190	+3·488	85986·908		
No. 11	Feb. 5th	M.	+7·10	86643·66	14173·8	50	283·477	29·920	69·91	0·495	0·110	86032·368	+0·041	+1·226	+3·560	86037·195	86037·54	86038·40
					16876·5	63	283·754	29·866	70·18	0·530	0·095	86032·962	+0·043	+1·208	+3·681	86037·894		
	Feb. 6th	P.	+7·12	86643·68	14784·5	52	284·317	29·856	70·16	0·585	0·130	86034·194	+0·057	+1·205	+3·672	86039·128	86039·26	
					16493·5* 16767·5 16768·0	58 59 59	284·369	29·828	70·52	0·565	0·103	86034·306	+0.049	+1·195	+3·834	86039·384		

Note.—Except where marked * I is the mean of three nearly equal periods.

In order to render these results comparable with those taken at other places they must be reduced to vacuum and sea-level.

The reduction to vacuum is

$$0{\cdot}34\frac{26}{1+{\cdot}0023\times 30}=26\times 0{\cdot}34\div 1{\cdot}069=8{\cdot}2693.$$

The height of the pendulum-bob above the sea-level is given by Mr. Russell as 140 feet.

The reduction to sea-level is therefore $\frac{140}{243}={\cdot}5761$.

The sum of the two corrections is 8·8454 or to a sufficient degree of approximation 8·85.

Hence we obtain as the finally reduced vibration numbers

Pendulum No. 4	...	...	86095·95
,, 6	...	...	85995·97
,, 11	...	...	86047·25

The values given by Mr. Baracchi for Melbourne are

Pendulum No. 4	...	...	86107·89
,, 6	...	...	86008·05
,, 11	...	...	86059·68

Hence we obtain for the difference $M-S$.

Pendulum No. 4	...	...	11·94
,, 6	...	...	12·08
,, 11	...	...	12·43
Mean ...	...	...	12·15 ± 0·19

Discussion of the Results, and Comparison with Theory.

The first conclusion deducible from these results is that the difference between the vibration numbers for Melbourne and Sydney cannot be deduced from a comparison of the investigations of the U.S. Coast Survey officers with those of Mr. Baracchi, for the difference between the value thus obtained, viz.:—8·58 ± 0·32, and the value 12·15 ± 0·19 given above, is more than eleven times the probable error of the first, and nineteen times the probable error of the second. It cannot, therefore, be attributable to unavoidable errors of observation.

Furthermore, the difference cannot be attributed to personal equation as between Mr. Baracchi and myself; for if I use my own (Melbourne) observations* instead of Mr. Baracchi's, I get nearly the same mean result, though with a larger probable error, $M-S$ coming out $12{\cdot}20 \pm 0{\cdot}47$; the difference 0·05 between the two values (which is less than the probable error of the determination made by either of us, and therefore within the limit of experimental error), is only one-seventieth part of the difference between the results of the two investigations at Sydney.

On the other hand the difference between the values for $M-S$ obtained by Lieutenant Elblein and myself is not extravagant, seeing that our apparatus and mode of experimenting are quite different; moreover, Lieutenant Elblein told me that he looked upon 1 in 100,000 as about his limit of accuracy for any one place; hence his limit of accuracy for the difference between two places would be about 1·7 vibrations per day. My own probable error for a similar difference is about 0·2 vibrations per day: hence the difference of 1·33 between Lieutenant Elblein and myself is well within the limit of experimental error.

We may therefore feel tolerably certain that, if we adopt 12·2 as the value of the difference between the vibration numbers at Melbourne and Sydney, we shall not be far from the truth.

If we compute by Clairaut's formula† the differences between the vibration numbers at Greenwich, Melbourne, and Sydney, and compare the figures thus obtained with the experimental values, we obtain some interesting results, which strongly bear out the deductions of the previous section.

The calculation is effected thus:—

Clairaut's theorem may be put into the form

$$V^2 = V_0^2 \left\{ 1 + \left(\frac{5}{2} m - e \right) \sin^2 \lambda \right\} \quad - \quad - \quad - \quad - \quad (1).$$

where V denotes the vibration number in latitude λ, V_0 the equatorial vibration number, m the ratio of the centrifugal force at the equator to the force of gravity there, e the ellipticity of a meridian.

* Referred to *supra* p. 1. There is, indeed, no reason to suppose that personal equation has any effect on the results of pendulum observations; the results for each station are themselves deduced from the differences between pairs of epochs, and as each epoch of a pair will be affected by the observer's personal equation to the same extent, this source of error is in all cases eliminated.

† It should be mentioned that—in order to avoid any risk of bias in favour of either Lieutenant Elblein's result or that of the U.S. Coast Survey—the calculations here given were intentionally not effected until the observations at Sydney had been completed and reduced.

Hence we obtain, if V_1 and V_2 are the vibration numbers in latitudes λ_1 and λ_2

$$\frac{V_1^2 - V_2^2}{V_1^2} = \left(\frac{5}{2}m - e\right)\frac{\sin^2\lambda_1 - \sin^2\lambda_2}{1 + \left(\frac{5}{2}m - e\right)\sin^2\lambda_1} \quad - \ - \ - \ (2).$$

m is known to be very accurately expressed by 0·0034674 ; the mean value for e obtained by Colonel Clarke from a comparison of all previous observations is 0·0034223 ; whence $\frac{5}{2}m - e = 0·00525$ with sufficient accuracy for our purpose. Owing to the smallness of this quantity we may omit the term depending on it in the denominator of the right-hand member of equation (2); furthermore, as $V_1 - V_2$ is small compared with V_1 or V_2 we may write the equation thus

$$2\frac{V_1 - V_2}{V_1} = \left(\frac{5}{2}m - e\right)\left(\sin^2\lambda_1 - \sin^2\lambda_2\right) \quad - \ - \ - \ (3).$$

For V_1 in the denominator of the left-hand member of equation (3) we may substitute 86000 without sensible error; and we obtain

$$V_1 - V_2 = 43000 \times 0·00525 \times (\sin^2\lambda_1 - \sin^2\lambda_2)$$
$$= 225·75(\sin^2\lambda_1 - \sin^2\lambda_2) \quad - \ - \ - \ (4).$$

the formula used in the computation.

The latitudes are as follows :—

Greenwich : $\lambda_1 = 51° \ 28' \ 31''$.
Melbourne : $\lambda_2 = -37° \ 49' \ 53''$.
Sydney : $\lambda_3 = -33° \ 51' \ 41''$.

The experimental values for Greenwich, Melbourne, and Sydney are summarised in Table IV.

TABLE IV.

Station and Observer.		Greenwich (Hollis)	Melbourne (Baracchi)	Sydney (Love)
Pendulum - -	No. 4	86165·19	86107·89	86095·95
	No. 6	86065·09	86008·05	85995·97
	No. 11	86116·83	86059·68	86047·25

From this table we obtain the following differences :—

TABLE V.

Pendulum.	$G-S$	$M-S$	$G-M$
No. 4 - - -	69·24	11·94	57·30
No. 6 - - -	69·12	12·08	57·04
No. 11 - - -	69·58	12·43	57·15
Mean - - -	69·31	12·15	57·16
Calculated - -	68·03	14·82	53·21
Obs$^{d.}$ – Calc$^{d.}$	+1·28	–2·67	+3·95

Hence, according to Clairaut's theorem the vibration number recently obtained for Sydney is too large as compared with that for Melbourne, and too small as compared with that for Greenwich; while the observed vibration number for Melbourne departs even more widely from that for Greenwich, being nearly four vibrations less than the formula would allow. Unless the pendulums have undergone some serious change in the course of the voyage out, such change being nearly reversed during the voyage to Sydney—a very unlikely concatenation of events—the conclusion to which the figures lead is a defect of gravity at Melbourne, and a similar but much smaller defect at Sydney, as compared with Greenwich. Setting aside any possible difference between the observed and calculated values due to the difference in longitude between Greenwich and the Australian stations,* we may observe that this result was exactly what we might expect. Melbourne being situated forty miles from the open ocean, and about 250 from the deep water marked by the 200 fathom line, is much more of a continental station than Sydney, which is near to the Pacific coast, and on a line with most of it, the 200 fathom line being here within a few miles of the shore; Greenwich, again, is on an island. The general result of pendulum work is to show that proximity to the ocean in continental stations raises the value of g for any given latitude, while an

* We know the figure of the Earth with sufficient accuracy to affirm that any effect arising from difference of longitude must be extremely small.

insular situation raises it still more: consequently $M-S$ should be smaller, $G-S$ greater, and $G-M$ still greater than the calculated value, as is the case.

If, however, we take the U.S. Coast Survey result, all three differences fall below the calculated value; $M-S$ in particular becomes absurdly small, unless we are prepared to assert that g has an abnormally high value at Sydney, owing to local peculiarites which cannot well be allowed for. Such an assertion appears to me to be quite unwarrantable in view of the agreement between the recent determinations of Elblein and myself; and I am in consequence reluctantly compelled to assume that the U.S. Coast Survey determination must not be employed differentially in connection with recent observations with the Kater pendulums.

What the source of the discrepancy may be it is not so easy to determine. The following suggestions may be made:—

(a) That some change has taken place in *all* the pendulums since 1883.

(b) That some error has crept into the reduction of the American observations.

(c) Errors of observing.

Of these *(c)* is practically out of the question, as all three pendulums give nearly the same result; besides the known skill and ability of the American observers would negative such a hypothesis.

(a) is inadmissible; for Herschel's measurements with these pendulums at Kew and Greenwich in 1882 agree very well with those made by Hollis and Constable in 1889, consequently no such change can have occurred during that interval; while the values of $G-M$, $M-S$, and $G-S$, as recently determined, negative the supposition of any serious change between 1889 and 1894.

(b) seems the only possible solution; and it is noteworthy that the error, whatever it be, affects all the pendulums alike. The possible sources of error which could give such a result are limited in number; the only one which suggests itself to me is a systematic change in the sign of the clock rate. Whether anything of this kind has occurred I cannot tell, as I have of course been unable to refer to the original notes of the American observers, which alone could be relied on to settle the matter.

Conclusion.

In the course of the observations, both here and in Sydney, a curious circumstance presented itself, which seems to indicate a peculiarity in the behaviour of the Shelton clock. Any *very rapid* change in the barometric pressure always calls forth *while it is progress* a change in the clock rate. As cases in point, the observations of 30th January and 5th February may be cited. On both these days the barometer fell rapidly during the first series of observations, and the value of V is on 30th January considerably higher for the second series than for the first, while on 5th February the reverse is the case. When, however, no sudden changes of pressure occur the difference between the numbers obtained on the same day is always quite small. Can this be a special property of gridiron pendulums? If so, the following explanation may be tentatively suggested. Possibly, owing to the friction between the rods, the geometrical form of a gridiron pendulum normally lags behind the condition proper to the actual temperature; and the sudden changes of pressure may act like slight shocks, enabling it to suddenly overcome this friction.

In concluding this paper I must express my gratitude to Mr. Russell, who not only placed the resources of the Observatory at my disposal, and aided me in the work in every possible way, but also treated me with the greatest kindness and hospitality during my stay in Sydney. My thanks are also due to the members of the Observatory staff, especially to Mr. Linehan, the chief assistant, who took a great deal of trouble over the clock rates, and helped me in other ways. Lastly, I would express my obligations to the New South Wales Railway Commissioners for their kindness in granting me a free pass over their lines.

Appendix.

On the Stability of the Stand on which the Kater Pendulums are swung.

The copper cylinder in which the pendulums are vibrated, together with the massive timber and iron framework which carries it, was constructed at Dehra Dun for the Indian Trigonometrical Survey. I cannot discover any record of experiments directed to discover whether the vibrations of the pendulums set up any corresponding vibration in the cylinder. It appeared desirable to investigate the question; and Mr. Russell very kindly constructed the apparatus here described, and assisted in the conduct of the experiments made with it.

The apparatus is figured in Plate I. *ABD*, Fig. (1),* is an L-shaped brass plate, to which two small plates *M*, *M*, Figs. (1) and (2), are soldered above and below. Through the plates *M*, *M*, screws *T*, *T*, Fig. (2), are passed, terminating in conical pivots which work easily in sockets in the brass plate *EF*; *G*, Fig. (1), is a weak spring fastened to *EF*, and bearing against the arm *AB* of the L-shaped piece, so that when left to itself the arm *BD* is pressed into contact with *EF*; to the arm *BD* a mirror, *C*, is cemented; *P*, Figs. (1) and (2), is a conical steel spike. The plate *EF* is secured by three screws to the wooden block *K*, which is itself screwed to the top of a large iron drum, *H*, filled with water, which stands on the floor. When in use the point of the spike *P* rested perpendicularly against the north window of the cylinder, against which it pressed with sufficient force to bring the arm *BD* parallel to the plate *EF*; it was found that a force equal to the weight of 0·25 ounces was required for this purpose. The plane of the thrust is parallel to the plane of vibration of the pendulum.

At the other end of the cellar a frame carrying a telescope and scale was supported by means of a similar iron drum filled with water, to which it was screwed, in such a way that an image of the scale was thrown into the telescope by the mirror; the telescope was provided with a single vertical crosswire. The arrangement of the telescope and scale is shown in Figs. (3), (4) and (5), and needs no further explanation. The scale was divided on ground glass into inches and tenths, and illuminated by a small bull's-eye lantern placed behind it on the frame. A displacement of one-tenth of a scale division in the image would have been easily detected, especially if oscillatory.

The dimensions of the apparatus were as follows:—Distance from point of spike *P* to vertical line of pivots, 0·75 in. Distance

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* Fig. (1) is horizontal, Fig. (2) is vertical.

from scale or object glass to mirror, 167·0 in. Distance from plane of spike to plane of support of cylinder, 43·0 in. An oscillation in the image of amplitude 0·1 scale division (0·01 in.) would accordingly correspond to an angular displacement in the cylinder expressed by

$$\frac{0 \cdot 01}{167} \times \frac{1}{2} \times \frac{0 \cdot 75}{43} \times \frac{648000}{\pi} = 0 \cdot 108 \text{ seconds of arc.}$$

To see whether the apparatus responded easily to small disturbances the following experiments were carried out:—

(a). To test the effect of unsymmetrical vertical thrust.

A brass cylinder weighing 13 oz. was placed in a vertical position on the ledges of the north and south windows of the cylinder alternately; the readings obtained were

No Load.		Load on S. window.		Load on N. window.
21·8	...	——	...	——
——	...	21·2	...	——
——	...	——	...	22·2
——	...	21·2	...	——
——	...	——	...	22·1
——	...	21·2	...	——
——	...	——	...	22·2
21·8	..		...	

[Note.—The pendulum cylinder is fully 100 times the mass of the small brass cylinder employed in this experiment.]

Hence the mean displacement on loading the S. window was 0·6, while the mean displacement on loading the N. window was 0·4; or, in other words, $2\frac{1}{2}$ oz. on the S. window, or 3 oz. on the N. window could be detected. As the line joining two of the three supporting screws of the cylinder ran east and west, and to the south of the third screw, this difference might be reasonably expected; for the load on the south window would tend to relax the pressure on the third levelling screw by rotating the whole cylinder about the E. and W. line through the other two.

(b) To test the effect of horizontal thrust.

The piece of apparatus shown in Figs. (6) and (7) was constructed for this purpose*; *abc* is a brass plate bent nearly at a

* Fig. (6) is vertical, Fig. (7) horizontal.